series for time in order to exchange with Berlin; but owing to a fault, afterwards found to be at Malta, no signals could be exchanged.

February 28. A splendid set of observations and exchanges with Berlin, the observations and exchanges lasting all night long.

March 1 and 2. Hopelessly cloudy, reduced the exchanges between Suez and Alexandria.

The results for Current Time are—

		Difference from Mean.
	s	s
From exchanges of 19th	+ 0·057	+ 0·027
„ „ 20th	+ 0·022	− 0·008
„ „ 23rd	− 0·011	− 0·048
„ „ 24th	+ 0·053	+ 0·023
Current Time	0·030	6

The probable error of the result will be very small, especially when we consider that no chronograph is used.

March 3. Low and I made two series of comparisons of personal equation; and between the series I made a series of observations with his transit to find if there was any difference in my personal equation in using the altazimuth and the broken transit.

(This was necessary in the connection of the two stations, Gill's and Hunter's, at Suez).

March 4. Low left for Malta, I for Alexandria.

March 5. Selected site for base line with General Stone.

March 6. Returned to Alexandria, exchanged signals with Berlin. Storm of wind and rain prevented observations.

Photography in the Transit of Venus. By R. A. Proctor.

I do not know whether Capt. Abney's remarks (p. 309) relate to a paper by the Chief of the Washington Observatory, which appeared in *Harper's Weekly Magazine*, or to an essay of mine quoting Prof. Newcomb's opinion in one of the quarterly magazines. Possibly to neither, as the great difficulty dwelt on by Newcomb is "placed on one side" in Capt. Abney's paper. It may be desirable to note the nature of that difficulty. The American Transit Committee, after many experiments and long inquiry, came to the conclusion that the diameter of the Sun, as depicted by the photoheliograph, could not be ascertained (at least with the extreme accuracy necessary for determining the solar parallax) either by calculation (depending on the optical

adjustments) or by direct measurement, or by any practical contrivance, such as photographing a scale. On the other hand, they consider that reliance can be placed on the calculated scale of a picture taken at the principal focus, while the centres of the disks of *Venus* and the Sun can be determined accurately, because each centering results, not from a single pair of measures, but from as many all round each limb as the observer may wish to make. Of course the position of the centres may be determined in the same way, from a photoheliographic picture; but no advantage results if there is no trustworthy scale of measurement. This, then, is the point at issue, viz., whether a trustworthy scale can be obtained. The available methods are (i) calculation, (ii) measurement of the pictured solar diameter, and (iii) photographing a scale. As to the first, I apprehend that there can be no comparison in point of exactness between the calculated scale of a picture at a principal focus, and that of a picture optically enlarged: it is only necessary to consider the optical adjustments and relations involved, to be assured of this. As to the second method, it matters little whether photographic irradiation be large or small; for at the lowest estimate ever yet made, the effects of irradiation must be fatal in such a problem as determining the solar parallax: apart from this, we now have evidence showing that the photographic Sun is really larger than the optical Sun. As to the third method, it seems sufficient to note that the use of a photographed scale introduces of itself a probable error as large as that in single measurements of the photographed disk of the Sun or *Venus*; and such an error would be fatal in a problem of this kind.

The fact pointed out by Capt. Abney that daguerreotypes "are subject to much greater fluctuations of expansion than are glass negatives," seems to counterbalance the superiority claimed for them (by Sir G. Airy, I think, at the January meeting) in point of definition.

It may be hoped that before the Transit of 1882 photographers and astronomers will have decided on some one method of photographing *Venus* in transit. The qualities of the various methods employed on the present occasion will be sufficiently indicated during the examination of the complete series of transit observations. One point seems already clear, viz., that contacts determined from photographic records differ from contacts observed with the telescope, the photographic Sun being larger than the Sun we see. Hence greater reliance will probably be placed on mid-transit photographs. This country is afforded a noble opportunity of serving science by providing Southern stations for this purpose in 1882; an opportunity of which it may be hoped that she will avail herself.

Worthing, 1875, *April* 3.